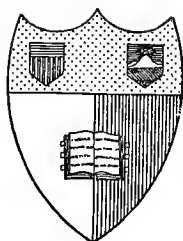


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THE PSYCHOLOGICAL BULLETIN

PERSONALITY AND CHARACTER

BY GORDON W. ALLPORT

Harvard University

Psychologists of the past decade have been much occupied with the application of their science to practical problems of life, particularly to problems of vocation and conduct. The social adjustments of the individual represented in such problems are receiving marked attention not only from clinical and consulting psychologists, but from experimental and research psychologists as well. The enticing topic of speculation, *human nature*, has been rephrased for scientific study as a problem of *individual nature*. On every hand we see that interest is becoming focused upon the study of man with the hope that by this method the problems of the mechanisms, motives, and modifications of behavior peculiar to the individual may at length be solved. This tendency is manifested by the rapid increase in the number of studies of human *personality* and *character*.

This paper is prepared with the hope that it may offer a useful summary of experimental and clinical researches, together with a review of the best of the speculative work, and so assist the reader to a clarified notion of the present status of psychological investigations concerning personality and character. For convenience the contributions to be considered will be arranged under three headings: (1) the psychological nature of character and personality (the problem of definition); (2) the composition of personality (the classification of traits); and (3) the measurement of traits by testing devices and rating scales.

I. THE PSYCHOLOGICAL NATURE OF PERSONALITY AND CHARACTER

In this field of study we may expect richer results from experimental and descriptive work than in the direction of definition.

Indeed, the problem of definition is almost hopelessly complicated by prevalent popular uses of terms. Recently, however, there have been noteworthy efforts made to delimit carefully the concepts of personality, character, and temperament as to their psychological significance.

Paton (44) considers personality to be the dynamic or functional aspect of individuality—"the progressive synthesis of the mosaic of experiences." Individuality which is mere organismal unity expresses only structural differentia between members of a species; whereas personality is in all cases the peculiar function of the individual. This view is similar to Kempf's (31) who considers personality to be the habitual modes of adjustment which the organism effects between its own egocentric drives and the exigencies of the environment. Between the autonomic system and the exactions of the physical and social environments we find the intermediation of the central nervous system, and the customary adjustments which this system makes between the egocentric drives of the organism and the requirements of the *milieu* constitute the personality of the organism. Such a standpoint serves to call attention to the biological fact that the primitive nervous system (the autonomic) is of immense importance in determining personality. The view as stated compels us to ascribe personality to all living things, unless we limit the concept of "habits of adjustment" to the social environment, leaving out of account the numerous adaptive responses made to non-social surroundings. Limiting the concept in this way would be to consider personality as developing *pari passu* with the number and complexity of social relationships.

There are many rag-bag theories of personality, into which are dumped "all biological innate dispositions, impulses, tendencies, appetites and instincts, and all acquired dispositions" (49). Such a conception partakes of chaos, for it does not give unity or definiteness of direction to our study. It is not difficult for the self observer to recognize that his own personality is something more homogeneous and dynamic than such a definition would suggest. Nor will the observer be content with the static conception that "personality is the totality of all our previous experiences and ideas." Hartenberg (22) introduces an organizing motive into the rag-bag. He considers personality to be the peculiar organic condition of the individual, with his individual attributes of sensory and motor behavior, capped and organized by a dominant sentiment or motive.

Myerson's book (40) seeks to analyze the foundations of personality, but does not attempt to distinguish it from character or

mind, since these are all alike viewed as functions of the organic activities of the body. It is to be regretted that the author despaired of defining his field a little more accurately, for there is much ambiguity entailed by confusing three such totally different problems.

The distinction between personality and character is an important one for psychologists to observe. Webb (64) defines character as the sum of all personal qualities which are not distinctly intellectual. In this regard he agrees with Fernald (15) who suggests that the formula for *personality* is *intelligence plus character*. McDougall (34) considers character to be the organization of sentiments, which are innate, into a system or hierarchy within the individual. According to this view character is not as unitary a function as Fernald would have us believe, nor can it be thought to be measureable. Gosline (20) considers character, along with intelligence and temperament, to be inborn, though susceptible to alteration by the environment. On this assumption character becomes a behavior function comparable with intelligence as a field for psychological investigation. In contrast to these views we find that of Watson (62). Character is defined by this author as the personality evaluated according to prevailing standards of conduct. Those psychologists who accept Watson's view have no right, strictly speaking, to include character study in the province of psychology; it belongs rather to social ethics.

Watson considers personality to be the sum total of an individual's responses to his environment, that is to say, his "reaction mass" as a whole. Kantor (30) distinguishes two types of behavior within the personal adjustment habits, (*a*) actions which are direct visible signs of the individual's nature, and (*b*) more permanent action elements, such as dispositions to action which may be considered as potential behavior. Such views as these, placing emphasis upon adjustment habits, have been at the base of the numerous attempts recently made to measure traits and to study the behavior patterns which constitute the personality. (Part III of this review).

II. THE COMPOSITION OF PERSONALITY

What traits do psychological writers recognize as the elementary constituents of personality? It has long been considered that traits are mental faculties, which might be studied by reference to the characteristic configuration of head or hand. It is not within the

scope of this paper to delve into these "slums of psychology": the reader who is interested will find profitable a review by Jastrow (27) of the antecedents of character study.

Psychologists are working steadily toward a schedule of human traits which will express in the most economical fashion a true analysis of personality. Wells (66) has gathered together the classifications of Heymans and Wiersma (23), Cattell (8), Hoch-Amsden (24), and Davenport (10), and has presented a synthesized schema of traits. The result is a list of fourteen types of adjustments which are fundamental to every personality. These essential modes of behavior are then divided into ninety-four traits which submit to quantitative study according to a rating scale. Partridge (43) and Yerkes and La Rue (71) further multiply the number of qualities, and so invite upon their schemes the criticism of superficiality and overlapping. In fact, a notable weakness at the present time is the tendency to make a hasty and inadequate selection of traits.

Spaulding (55) represents one of the many recent attempts to find a practical schedule of traits for application to a special problem—in this case, the problem of delinquents. The study presents one hundred thirty-four qualities arranged under eight principal divisions of personality (intelligence, emotionality, and the like). Porteus (46, 47) has made a similar attempt to discover the qualities which are essential for the social adaptation of defectives. Business leaders, social workers, educators, moral directors, are continually publishing personal analysis charts. The same criticism can be made of one and all: the traits chosen are not psychologically basic; they represent complexes of habits (neatness, tact, and the like) rather than truly fundamental aspects of personality. Woodworth (70) presents a plan of four subdivisions, physique, chemique, instincts, and intelligence—more suggestive than the four humors upon which most of the French studies of the *caractères* are based—but still very schematic.

If we turn our attention from the classifications *in toto* to some of the special traits which are frequently advocated to be elementary constituents of personality, we may then be able to see the problem more clearly. It is dangerous to oversimplify the problem of classification, and in the discussion which follows it must be remembered that by no means all of the alleged components of personality are included. The traits chosen are merely those upon which a large number of students of personality are focusing attention at the present time.

Intelligence.—No scheme of personality denies a signal position to the intellectual capacity. Terman (57) and Webb (64) find that high intelligence correlates in general with all strong qualities of character. However intelligence may be defined, there can be no doubt that the mental adaptability for which it stands is one of the major factors in the development of personality.

Temperament.—The emotional characteristics of an individual are likewise accorded universal recognition. There is, however, little agreement as to what temperament is. The four-humor doctrine has haunted the house of psychology for twenty-five centuries. The advent of endocrinology, as Paton (44) points out, has done much to supply a reinterpretation of the theory, and as the work progresses we may find that the anticipations of Hippocrates and Galen were not so far afield. There has been a steady increase of emphasis upon the rôle which the ductless glands play in determining the emotional life. Berman (4) has recently published a mechanistic interpretation of personality almost entirely in terms of glandular influences. He speaks of the thyroid personality, the pituitary personality, and the gonado-centric personality, according as these autocoid influences seem to predominate in the individual!

Davenport (9) suggests that qualities of emotionality, e.g., a "hot temper," may be inherited according to the Mendelian principle. Heymans and Wiersma (23) are quite certain that their findings based upon a *Massenuntersuchung* show evidence of the positive inheritance of traits of character and temperament. Gates (19) has reviewed the current beliefs concerning the inheritance of mental traits in this journal.

Volitional Qualities.—Downey (12) has called attention to the significance in personality of the factors of activity and control. Myerson (40) employs this conception of the "kinetic" nature to describe personality types which seem to be characterized by a certain speed or intensity of response. Prominent among these "volitional" types we find the ascendant personality and the submissive personality, characterized respectively by habitual aggression or passivity. The analogy of the active and passive voice in these types was first suggested by Southard (54). Allport (2) has applied this conception to the problem of salesmanship, showing that timidity in the selling personality is essentially passivity, and this is to be overcome only by a deliberate building up of one's ascendant qualities. Ascendancy is the essential trait of leader-

ship. Gowin (21) has found it to be correlated with physical attributes of size, weight and personal appearance.

There are other characteristics of the personality which are of great importance, but which have not been adequately studied. Such, for example, is the æsthetic nature. Again, we have the expansive personality as contrasted to the recluse. There has been no study of these traits to the present writer's knowledge, excepting his own research (3) in the Harvard Psychological Laboratory. (See Part III of this paper.)

Reference should be made to two or three notable contributions from psychiatrists and Freudian psychologists. White (69) considers conflict in the psychoanalytic sense to be the cardinal problem of character formation. The well-balanced personality is the one which effects a successful "resolution" of all conflicts. Adler (1) has advanced the theory of *Minderwertigkeit*, according to which each individual who finds himself handicapped in life's struggles because of physical or mental defects will tend to compensate for this consciousness of personal inferiority. The inherent desire of a person to be superior and powerful leads him to make many peculiar attempts at adjustment, to preserve at all costs his "feeling of masculinity," and the characteristic habits which result are true traits of personality. The biologist, Le Dantec (32) likewise holds egoism to be the determinant of all actions, and the individual's personality merely represents the "deformations" which *la vie en commun* compels him to undergo. Adler's theory has been interpreted and popularized by Tanner (56), Myerson (40) and others, until Compensation has become one of the psychological headlines of the day.

Jung (28) has drawn attention to two important types of adjustment. The extravert finds his real life one of keen participation in his environment, the introvert on the other hand living much within the world of self-consciousness and fantasy. These types, those whose adjustment is realistic, and those whose adjustment is autistic are considered in detail by Wells (65). Day-dreaming he holds to be one of the characteristic attempts at inner adjustment when outer adjustment fails.

Rosanoff (50) gives an excellent account of psychopathic personality types. There are individuals whose personality might be characterized as (1) antisocial, (2) cyclothymic, consisting of manic make-up, depressive make-up, irascible make-up, or emotional in-

stability, (3) autistic, *i.e.*, predominantly introverted, and (4) the epileptic personality, with its periodic alterations of mood and character. Normal personalities are considered by Rosanoff to differ from the psychopathic only slightly as to quality but markedly as to quantity. The schizophrenics and epileptics, it is true, evidence brain atrophy, but in regard to most traits there appears to be a perfect continuity of type from the most normal individual to the totally insane. Southard (53), has made a suggestive innovation by the application of grammatical categories to personality study. Certain features of the personality, he says, may be studied with profit by describing them in the terms of syntax: person, number, mood, voice, and tense.

III. THE MEASUREMENT OF PERSONALITY BY RATING AND TESTING

In the field of personality measurement we find an encouraging amount of meritorious research. In all probability, as has been the case with the study of intelligence, we shall be able to give reliable quantitative results before we understand the precise nature of that with which we are dealing. Even before the days of Freudian-endocrine-psychiatric enlightenment as to the probable nature of personality, Francis Galton definitely advocated psychometric tests for traits of character. He suggested, for example, that it might be profitable to construct a delicate apparatus for recording individual peculiarities of manner in sitting upon chairs. Such a device would most certainly yield important information concerning the sitter!

1. *The Rating of Traits.*—Norsworthy (42) seems to be a pioneer in the study of the validity of judgments upon character. Her conclusions, substantiated in the main by subsequent investigations, are (1) that the reliability of rating varies with the traits under consideration. Raters agree more closely upon such qualities as popularity, conceit, or leadership than upon emotionality, honesty, or tact. (2) It seems also that some individuals are easier to rate and their rank more readily determined by the raters than other individuals. We have thus a suggestion of the open personality versus the enigmatic. Cattell (8) decides that traits upon which the judges closely agree represent the individual's reactions to objective things, whereas the traits upon which they disagree most represent the individual's reactions toward other people.

A study made by Thorndike (59) sounds a warning which all dealers in rating scales may well heed. It was found in his study that those giving ratings were unable to analyze out the different aspects of an individual's nature and rate them independently of other aspects. The ratings are affected by a marked tendency to think of the person in general as good or rather inferior, and so color the judgments of the qualities by a general feeling. Thus a "halo" is provided the individual who is being rated. Moore (36) finds evidence of this halo as an influence upon the rating of college students by members of the faculty. This same source of error helps to account for the conclusions reached by Rugg (52) that the rating scale used in the U. S. Army (45) probably did not locate an officer within his "fifth" of the entire scale. The scale is reprinted in this journal, and a discussion of the theory and applications is presented in the same number by Terman (58). Rugg maintains that to render rating of human character practicable, the following conditions should be fulfilled: (1) The final rating must be the average of three independent ratings, each one made on a scale as objectified as the man-to-man-comparison type of scale. (2) The scales on which ratings are made must be comparable and equivalent, having been made in conference under the instruction of one skilled in rating scale work. (3) The raters must be so thoroughly acquainted with the person rated that they are competent to make the judgment. The question of reliability has not deterred schools and industries from using rating scales, and whichever way we turn we meet with these schemes (41, 60, 63). Wells (66) and Spaulding (55) endeavor to eliminate the difficulties of scoring by the employment of a simple + and - designation opposite each trait in their scales, indicating superiority or deficiency of the attribute as compared to the average or normal person.

Self-rating is fraught with perils. Hollingworth (25) finds that the individual generally tends to overestimate his possession of qualities which are socially valuable and to underestimate in the case of those which are socially undesirable. Thus, no person would be content to ascribe to himself anything but a superior possession of refinement or humor. Hollingworth found it to be a rule that the individual's deviation in self-ranking from the place accorded him by averaging the ratings of his associates is noticeably greater than the average deviation among the judgments of the associates themselves. The possibility should be noted that the

extent to which the self-rating deviates from the average rating by associates may be used as an index of the insight of the person studied. The lower the average of his deviations from the rating accorded him on various traits, the higher his insight. In cases where ratings are made on desirable or undesirable qualities a Self-evaluation or Conceit Index might be computed in a similar fashion.

Notwithstanding the dangers and difficulties encountered in devising and employing rating scales, we are forced to recognize this method as the only available objective criterion of personality. The sources of error must gradually be overcome by the improvement in the technique of rating. The methods of measuring objective manifestations of personality which we shall now turn attention to, are as a rule more unreliable than ratings by associates. The only method of judging the value of a particular test is by its correlation with ratings. Since both factors are generally so variable, we cannot expect more than very modest correlations from even the most refined test methods. There is cause for optimism, however, in the keen and original methods which are being devised for obtaining objective proof of the presence of a given trait and for approximate measure of its magnitude.

2. *The Testing of Traits.*—The development of the age and point scale standards for the measurement of intelligence has made clear to psychologists the imperative need for reliable methods of studying other aspects of personality. To supplement the I.Q. we need reliable measures of emotional, volitional and social qualities. Brubacher (5) suggests the concept of a Personality Quotient, and applies his idea to the qualities requisite to the teaching profession. It must be remembered, however, that differences of personality are of a qualitative as well as of a quantitative sort. This difficulty stands in the way of the development of a measurement scale based on the correlation between tests and familiar objective criteria such as those of intelligence. Methods which have been employed aim for the most part at a sampling of customary reactions, and the results obtained contribute rather more to a descriptive treatment of personality than to a quantitative analysis.

The Word Association Method.—Jung (29) brought to our attention the possibility of studying egocentric and objective types by their characteristic responses in the word reaction experiment. Wells (68) has published a bibliography of German studies which have dealt with the relation between the association method and

the study of temperament. The same author (67) has made a study of the egocentric and objective types, with the discovery that the former tends to give the more unusual responses as measured by standardized frequency tables. Complex types of introversion are studied in reference to diagnostic criteria by Hull-Lugoff (26). In this study it was found that the most reliable single indicator of the tapping of a complex is the tendency of the subject to repeat the stimulus word. Other specially valid criteria are a prolongation of reaction time (over 13/5 seconds), inability to make any response whatever, and extremely short reaction times. The findings of Dooley (11) show that long reaction times in general seem to go with the egocentric type, and that the habitually quick reaction is a sign of objectivity in the reagent. In an experiment with directed egocentric reactions, Washburn and others (61) assumed that subjects who can make a required personal association with the least delay are egocentric. The present writer in a recent experiment has not found this assumption to be justified.

Moore (35) uses the association method to test the strength of instincts. Stimulus words are arranged into constellations according to McDougall's schedule of instincts. The subject is asked to make a personal association which is scored on the basis of indicating strength or weakness in the possession of the instinct in question. Morgan and others (38) presented stimulus words to subjects for five successive mornings, and secured the response from them as to whether the words were pleasant or unpleasant in tone. An index of optimism or pessimism was thus computed which showed a certain correlation with the ratings on this trait by associates. Not unlike this method is the study of personality through an analysis of the content of images by Martin (33). Mental and physical peculiarities and preferences are considered to be subject to profitable study by this method.

Measurement of Motor Expression.—The relation of motor impulses to psychic patterns has suggested several experimental attacks. Much work has been based upon the theory that the *petites gestes* involved in handwriting are movements as expressive of personality traits as are gross bodily movements. The work of Klages, Binet, Crépieux-Jamin and other graphologists has been summarized by Downey (14). This author herself made a careful study of handwriting and concludes that it bears certain definable relations to character. Some of the types to be studied by this method are the Explosive, Obstructed, Sensory, Motor, Hyper-

kinetic, and Hypokinetic. A careful refinement of technique in the study of handwriting is necessary, for gross features are often misleading. "The broad classification of outgoing movements and movements of withdrawal as characterizing respectively attitudes of aggression and of defense cannot sustain too great a weight." Downey (12, 13) has designed and partially standardized a Test for Will-Temperament. Numerous factors such as speed, size, disguise and control in handwriting are measured carefully and weighted. The scores for the various portions of the test may be plotted into a Will-Profile. Bryant (6) in a study of delinquent boys has shown that by Downey's method it is possible to secure an approximate measurement of the volitional qualities which make for social or antisocial conduct. Ruch (51) however, finds that the actual correlations between the scores on the Will-Temperament Test and student and faculty ratings are low. The Carnegie Institute of Technology (7) has adapted the Downey test for practical use in business, one of the many indications of the current enthusiasm for applying psychological methods.

"Will power" is studied by Fernald (16) by means of an apparatus for measuring the ability of the subject to stand upon his toes for a long period of time. The problem of the shifty eye in relation to the lack of aggressiveness has been studied by Moore (37), with the conclusion that, taken together with certain other diagnostic signs, the number of eye movements noted while the subject performs a given mental problem is indicative of his ascendant or submissive attitude in general.

Represented Situations.—One of the methods of testing character dispositions definitely advocated by Galton is that of representing in miniature certain problems of actual life, and of observing the individual's adjustment to these situations. Freyd (18) employs representative problems in news getting as one of a series of nine tests for journalistic aptitude. Fernald (16) presents to a delinquent subject ten questions concerning ethical interpretations, and finds that the ability to make a discrimination according to the conventional code in the case of these samples is an approximate measure of the subject's ethical adjustment as a whole. Allport (3) has used this method as a test of ascendance-submission, and finds that the scores on sample problems representing the individual's behavior in situations which would require his taking an active or a passive rôle, correlate slightly with personality ratings by associates on this trait ($r = .40$). Myerson (39) suggests limiting

the number of possible answers to a miniature problem, say, to four. This "multiple choice" device facilitates scoring, and suggests at once which of four possible types the reagent belongs to. Ethical discrimination and humor are the traits which Myerson has most extensively studied by this method. Pressey (48) has published a test for emotional spread. Lists of mixed words, strong and weak, are presented to the subject who is required to cancel out all which have unpleasant feeling tones associated. The "affective spread," *i.e.*, the range of situations to which the individual is wont to react in an emotional manner, is a distinctly serviceable concept. Pressey's study stands out as practically the only attempt so far made to measure the complex emotional life of the individual by a simple testing device.

Questionnaires.—The questionnaire method of studying personality has, with all its limitations, definite value in checking on the results of tests, and for suggesting traits which otherwise might escape notice. One of the revelations, for example, which can be made by the answers to a questionnaire concerns the factor of compensation. This important mechanism defies direct experimentation, and can be revealed only by a historical study of the person such as a carefully devised questionnaire affords. Perhaps the completest lists of questions are Watson's (62) and Woodworth's, the latter to be found in Franz's handbook (17). Both of these questionnaires have the advantage of being couched in "behavioristic" language, and the latter lends itself to simple "yes" and "no" responses. However highly this method may be developed, as an instrument for accurate study of the personality it will always face the weakness of permitting falsification or rationalization on the part of the subject.

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MENTAL WORK¹

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A sharp line of distinction between mental and physical work is, as one might expect, still undefined. Thorndike (81, pp. 1-7) not without some hesitancy and reservation has suggested that mental work is that performed by the connection system. Watson has subjected this statement to criticism (92, p. 350). According to his own suggestion, what we commonly think of as mental work differs from physical work in regard to the muscle groups employed. Mental work is for him mainly laryngeal work.

Thorndike (81) and Dodge (24), although approaching the problem from essentially different angles and although differing very much in their modes of experimentation, have a good deal in common in their contributions to the theory of mental fatigue. Thorndike² has divided theories of mental fatigue into two classes (pp. 119 ff), the mechanical and the biological. According to the mechanical theories, mental fatigue is brought about by the consumption of a stock of potential energy. Such theories he objects to largely on the grounds that efficiency losses as shown by the mental work curve are, because of their extreme irregularity, more easily interpreted in terms of such factors as changing interest, distraction by headache, and the like than in terms of energy consumption in its simple sense. According to the biological type of theory, which Thorndike favors, work without rest decreases in efficiency largely because "it becomes less satisfying (1) by losing the zest of novelty, (2) by producing ennui, a certain intellectual

¹ This review will discuss developments in the field of mental work from 1912 to the present time. Papers concerned mainly with physiological aspects of work have not been considered. Undoubtedly the psychologist will find much in them that is interesting and valuable, but a review such as this must stop somewhere. Studies of work in industry are usually concerned with operations, which, if we make a common sense distinction, are muscular rather than mental; for that reason they have not been considered in any detail. On the other hand, where psychological investigations have included data on obviously sensori-motor processes, these data have been considered. The material reviewed here, while not selected with any too rigorous consistency, does represent a fairly unified line of study.

² Thorndike's conclusions here are based upon his researches dating back to 1900.

nausea, sensory pains and even headache, and (3) by imposing certain deprivations—for instance, from physical exercise, social intercourse, or sleep” (p. 122). Dodge has met the energy-consumption theory of mental fatigue in a manner not unlike that of Thorndike. Although in an earlier paper (23) he recommended an attempt to measure amount of mental work in terms of a metabolic unit, his interest has apparently changed somewhat. He now insists upon the importance of *relative fatigue* which “is not a mere limitation of human energy. It is not exhaustion, but prevents it” (p. 112). He thinks it unlikely that ordinary decrements in the mental work curve are results of true fatigue processes in nervous tissue. They are more apt to be due to incidental inhibitions. The views of both Thorndike and Dodge have put a wholesome emphasis upon the complexity of the causes underlying fluctuations in the curve of work. They have shown quite distinctly what *mental fatigue* is not and they have suggested the importance of studying not only the decrements in efficiency of given functions, but also the many forces, incentives, or what you will, accelerating those decrements.

Whereas Thorndike and Dodge have tried to give analyses of the phenomena commonly known as mental fatigue with a view toward correcting the false analogies drawn between it and fatigue in the narrower, physiological sense, Watson (p. 349) has recommended that the concept of fatigue be banished from psychology. Evidently he does not mean that loss of efficiency caused by continuous work should not be studied, for he treats this subject at some length. Evidently, too, he does not mean that such a question as that of incentives should be left out of account, for he suggests that every ascertainable aspect of the situation in which work is done may well be noted. His main quarrel, it seems, is with physiological, or perhaps he would say synaptic, theories of efficiency. “The point we would make in doing behavior work,” says Watson, “is to leave out of discussion all those factors which are not touched or approached by the problem in hand; for example, what possible good does it do when discussing bricklaying or subvocal arithmetic to guess at what goes on in the synapse, in the efferent or afferent leg of the reflex arc or in the muscle itself?” (p. 352). This line of attack upon the customary treatment of mental fatigue differs from those of Thorndike and Dodge. They criticize the physiological theories of mental fatigue because they feel that these theories are erroneous or, at least, incomplete. Watson, apparently, criticizes them not so

much because they are erroneous or incomplete as because they are physiological and because they are theories.

Muscio (66) quite recently has again pointed out the fallacy of calling all decrements in efficiency growing out of work effects of fatigue. Like Watson, he would banish the term from scientific discussion.

Methods of measuring fluctuations in mental efficiency have not undergone any very radical changes during the past decade. Investigators of the nature of the work curve have naturally employed the *continuous work* method. Most of their work has been done with arithmetical activities. Investigators of the effects of drugs, hour of day, loss of sleep, fasting and the like have usually employed the *testing* method. In general there has been a decline in the use of such tests of efficiency as esthesiometry, sensory acuity and spontaneous tapping rates.¹

While no radically new methods have been introduced into this field, there has been a constant refining of technique. Greater care has been taken to secure adequate controls over the attitudes of the reagents studied. Rivers, somewhat before the period covered by this review, made much of the faulty methods of most of the earlier investigations of fatigue, the effect of drugs, etc., and experimenters during the past ten years have taken many new precautions against the vitiating influences of psychological attitude.

Proposals to measure amount of mental work done in terms of some physiological change are not new. There have, however, been some rather interesting suggestions along this line. Dodge (23), for example, has pointed out the close relationship between pulse rate and general metabolic conditions and has suggested pulse rate as a measure of mental work done. His paper on this subject contains some data on the relationship between pulse rate and the performance of a variety of intellectual tasks. Urban (90) criticizes Dodge's program on the grounds that even if pulse rate were an exact measure of metabolism, only the bodily accompaniments of mental work would be measured by it. Such a criticism could not be answered except on metaphysical grounds. Lehman (54) also desires some measure of mental work in physiological terms. He believes that mental fatigue is accompanied by an increased amount of exhaled CO₂.²

¹ For a discussion of these tests as measures of mental fatigue, see: OFFNER: *Mental Fatigue* translated by G. M. Whipple.

² The reader may recall the work of Benedict, Atwater, and Carpenter on this point.

Another important paper from a methodological standpoint is a recent one by Thorndike (84) on the curve of satisfyingness. Here emphasis, which has during the past ten years been everywhere else, turns back again upon "feelings of fatigue," or at least upon data obtained from purely subjective observations. The subjects of this experiment periodically rated the "satisfyingness" of a continuous task.

THE CURVE OF WORK

Perhaps the most important paper on the curve of work from a critical and theoretical standpoint is one of Thorndike's (85). He attacks the analyses which Kraepelin and others have made of the course of efficiency during continuous work. As a result of their analyses a number of features or elements stand out as characteristic fluctuations in efficiency. These are practice effect, fatigue effect, "warming up" effect (*Anregung*), adaptation (*Gewöhnung*), initial spurt, end spurt, spurts after fatigue (*Ermüdungsantriebe*), spurts after disturbance (*Störungsantriebe*) and the rhythm of attention. Thorndike is inclined to believe that these features of the curve of work have been much overemphasized. "The most important fact about a curve of efficiency of a function under two hours or less continuous maximal exercise is that it is, when freed from daily eccentricities, so near a straight line and so near a horizontal line" (p. 188). It seems to be his main contention, however, not that warming up, initial spurt, and the like are fictitious, but that they are conceptions which should be empirically investigated, rather than conceptions which can profitably be used as principles explanatory of work curve irregularities.

Chapman (16) found clear evidence for an initial spurt in continuous adding. In a later paper by Chapman and Nolan (19) it appears that initial spurt is more marked where the work is divided into half-minute periods than where it is divided into two-minute periods. This suggests that initial spurt may be so short in length that it can easily be concealed where average scores for relatively long successive periods are considered. Phillips (71) obtained evidence for an initial spurt in a ten-minute period of arithmetical work. During the first minute his subjects performed from 6 to 12 per cent. more efficiently than they did, on the average, during the remaining 9 minutes. Thorndike (83) has also published some positive evidence in regard to initial spurt (his subjects wrote from memory or with reference to a key answers to multiplication problems). For a minute or so at the beginning of the work the

subjects maintained a speed which they soon abandoned. Thorndike does not interpret this as evidence of a general fact of initial spurt. He thinks that, since the earliest work is not immediately preceded by any work like it, it is, therefore, less open to confusion effects than the later work.

"Warming-up" also seems to occur under proper conditions, *i.e.*, where each day's work is in a sense a learning process. Starch and Ash (79), for instance, have results on two and one half hours of adding which indicate a rise in efficiency as measured by both speed and accuracy during the first 25 to 35 minutes. End spurt, too, it is generally admitted, may occur when conditions are proper, the *sine qua non* being knowledge on the part of the subject as to when the work is to cease.

A number of studies have contributed to our knowledge of the amount and nature of the losses in efficiency due to the continuous operation of various specific functions. Arai (3) performed mental multiplications (four-place by four-place numbers) continuously for as long as twelve hours. At the end of such a period it took her 100 per cent. longer to do one problem than it had at the beginning of the period. Groups of subjects engaging in such work for two-hour periods took 24 per cent. longer per problem at the end than at the beginning of the period.

Garth (32) found that children, adding for 28 minutes or 42 minutes, suffer an efficiency loss of from 0 to 5 per cent. Thorndike (82) discovered that the loss from four hours of poetry writing is a matter of quality and satisfyingness rather than of quantity of work done. Painter (67) shows that one's ability to work may give out suddenly. After a hard day's work he multiplied mentally four-place by four-place numbers from 11 p.m. until 3 a.m. At the end of the period he was unable to continue this work. He was also unable to perform easier multiplications. Vernon (91) claims that among women workers in English munition plants the 12-hour day caused 2.5 times as many accidents as the 10-hour day. If this conclusion is sound it is another illustration of a sudden acceleration of the fatigue process. Poffenberger and Tallman (73) suggest that mental fatigue may proceed at a rate obscured by the conditions of most experiments. They found that the first half of a test, the total length of which is less than one minute, is done more quickly than the second half. But the repair process may also be so rapid that unnoticed periods of relaxation within the ordinary work period may hide an actual alternation of fatiguing and recovering.

The extent to which the continuous operation of a function affects its efficiency is a matter not only of the function involved but also of the method of testing it. Morgan (63) had subjects spend four hours at studying the English equivalents of 850 German words. Much more of the material studied early in the sitting was recognized at a subsequent test than of that studied late. Learning scores and relearning scores on the other hand show no such clear distinction between the efficiency of the earlier and later parts of the study period.

PHYSIOLOGICAL EFFECTS OF MENTAL WORK

The effects of so-called mental work upon so-called physiological activities are still far from being worked out to any complete extent. In 1913, Dodge, as already indicated, reported certain correlations between variations in pulse rate and the performance of certain kinds of tasks. Arai's results (3) show a decrease of pulse rate with long-continued mental multiplication. Patrizi (69) found some correspondence between volumetric changes in the arm and auditory stimulation. These changes seem to be controllable by some subjects. According to another experiment performed by Patrizi (68) it appears that the ergographic work of some subjects is benefited and of others interfered with by an accompanying mental task. Zondek (96) has published a paper in which he shows that a brief period of writing fatigues the muscles of the legs. There is some doubt as to the degree to which writing should be considered a mental task. The results which I have mentioned are typical of those in this particular field. As far as they go they are definite enough, but their interpretation and explanation is well nigh impossible largely because the mental activities with which we are interested in dealing are not at the present time susceptible to anything like a complete analysis from a physiological standpoint.

SUBJECTIVE EFFECTS

Reports of *feelings of fatigue* have for the most part come as by-products of studies concerned mainly with more objective data. Arai (3), for instance, speaks of a positive but slight correlation between feelings of fatigue and production records. G. E. Phillips, (quoted in 65) obtained a similar result. Ioteyko (41) makes the point that sensations of fatigue, like pain sensations, do not follow Weber's law. In these realms sensation values increase more rapidly than stimulus values. Robbins, Smith and Washburn (75),

following up an earlier Vassar experiment, show that a prolonged series of judgments on the affective values of colors tends to lower the affective values of colors for the subject judging. Berliner (10) tested the ability of subjects to hold memory images. The experiments were conducted in the morning before the day's work and in the evening after the day's work. Her subjects held the images considerably longer at the morning sittings. Thorndike's paper (84) on the curve of satisfyingness has already been mentioned. In one experiment five subjects worked at the same task for one sitting of four hours. At the end of successive twenty-minute periods the subjects rated the satisfyingness, tolerability or zest of their task on a scale ranging from 0 to 10. According to the results one becomes bored with a continuous task at a much greater rate than one loses in speed or accuracy of performance. Muscio (65) reports the estimates of their own "tiredness" supplied at different hours of the day by a group of 20 women medical students and by a group of 15 women typists. In general the diurnal curves fall as the day advances, but for both groups there is a sharp rise in feeling tone just after luncheon.

EFFECTS OF ONE FORM OF MENTAL WORK UPON ANOTHER

Since rest is at best a term of only relative significance, the problem of the effect of rest upon work is, when more exactly formulated, a problem of the effect of one form of mental activity upon another. Dockeray (20) studied the effects of physical work upon mental work with very irregular results. Arai (3) states that the degree to which loss of efficiency due to the continuous exercise of one function operates deleteriously upon other functions is dependent upon the extent of loss in the first function.

More definite findings appear in the studies of Ash (6), Chapman (17), and Fernberger (29). Ash found that adding has a marked influence on one's ability to control reversible perspective. He avoids an interpretation of this fact in terms of consumption from a common energy supply by assuming that it is not the amount but the control of energy which is affected. In Chapman's experiment it was found that half-minute periods of adding were not as efficient when continuous for three minutes as when half-minute periods were alternated with cancellation or rest. He explains this fact in terms of interference. According to Fernberger a half-hour period of translating difficult German has no appreciable influence upon the judgment of lifted weights. Where dynamometric or ergo-

graphic work was done with the muscles used in lifting, however, there was a considerable effect. The interval of uncertainty, for example, increases by 30 per cent. and there is a marked tendency for the second weight to be overestimated.

REST PAUSES

Considerable work has been done on the optimal distribution of exercise for habits still in the process of formation, but little that the reviewer is aware of has been done on the optimal distribution of effort for fully acquired habits. This is true at least for habits which would ordinarily be called mental. The effects of various distributions of ergographic work have been studied and some attempts have been made to establish optimal work and rest periods for industrial workers. Jones (46) describes two investigations of this sort. "Mr. C. E. Knoeppel started with workmen who, at their own gait, had been producing 16 pieces per hour. By establishing a 25-minute working period and a 5-minute rest period, he obtained 18 pieces. By changing to 17 minutes of work and 3 minutes of rest, the output rose to 22 pieces per hour. Finally by arranging a 10-minute work period and a 2-minute rest period, production became 25 pieces. In another case a record of driving 1,600 rivets per day was obtained, the previous performance having been 600 per day, Mr. H. F. Stimpson establishing rest periods of 2 minutes between each 10 rivets, thus devoting 320 minutes, or 5 hours and 20 minutes out of the 10-hour day to rest, and employing a schedule of 1 $\frac{3}{4}$ minutes of work and 2 minutes of rest" (p. 222). Similar investigations would probably yield as interesting results for the so-called mental types of work.

In 1913, Dodge (22) reported some experiments on the wink reflex which might well be mentioned here. In disagreement with the earlier claims of Zwaardemaker and Laws his results show no absolute refractory phase for this reflex. As two stimuli (sounds) are presented closer and closer together in time, the percentage of double reactions gradually becomes less.

DISTRACTION

Distraction has been studied mainly by the reaction-time method. As a result of the work of Cassel and Dallenbach, Evans, Morgan, and Todd, a number of interesting facts and problems have been uncovered. Distraction may increase or decrease the speed of reaction, depending upon the temporal position of the

distraction (15, 88). For example, distractors given before the reaction stimulus may increase reaction time, while the same distractors given with the reaction stimulus may decrease reaction time (88). Apparently the most effective distraction is sound. The effectiveness of a distraction is also greater if it is of the same sensory modality as the reaction stimulus (28). The power of a given distraction may be reduced by increased effort on the subject's part (63), or by a process of habituation (15, 28). On the whole, continuity of the distraction favors adaptation to it (15). When once a given distraction has been adapted to, that adaptation will hold over to some extent even to conditions where the stimulus to reaction is changed (28). Certain of these findings are true in an undoubtedly wider sense than others. All of them suggest problems and hypotheses in regard to mental work in general.

INCENTIVES

The importance of incentives is pointed out again and again in studies of mental work. Investigators of the effects of drugs, ventilation and the like have endeavored to control these factors as well as possible. The usual procedure has been that of supplying the maximum of incentives practicable under the circumstances of the experiment. This has been done by directing the subjects to work as hard as possible, by giving them access to their records and in a number of similar ways (see, *e.g.*, 80). Writers on industrial work of various types also make plain the importance of incentives, but although they occasionally offer empirical data, it is usually too complicated to invite any confident interpretation.

Straightforward studies of the incentive problem, as such, are rare. Arps (5) found that subjects do ergographic work somewhat more efficiently when aware of their records. Chapman and Feder (18) found that school children do better in certain tests when offered prizes and when made aware of their results, and Allport (2) found that even on simple tasks subjects are influenced by their social surroundings.

It is natural to wonder why such obviously important factors in efficiency as incentives have not been subjected to thorough experimental investigation. The answer lies in the extreme complexity of the incentive problem. There are probably as many incentives affecting the quality and quantity of mental work as there are kinds of likes and dislikes in human life. And every student of the subject recognizes that these are not only hard to

isolate, but also hard to define. Rivalry, a very evident incentive, is one which can be experimented with in a way, but an investigator working with such a factor would require a great deal of insight, or at least of hardihood, to interpret his results.

LOSS OF SLEEP

Smith (78) carried out a rather elaborate investigation on the effects of loss of sleep. Five tests were employed: (1) the McDougall dotting machine, (2) an associated words test, (3) reversible perspective, (4) tapping, and (5) a nonsense syllable test. Following a period of insomnia the subject showed an increased capacity for tasks requiring concentrated attention, *i.e.*, the dotting machine and associated words tests. Later a period of lowered efficiency set in and records remained below normal for some days. A further important finding is brought to light by the results of an experiment where a period of insomnia was interpolated before efficiency, reduced by a previous period of insomnia, had returned to normal. Fatiguing the subject who was already in a fatigued state temporarily increased her efficiency. Subsequently, however, there set in a prolonged period of subnormality. Miss Smith suggests the possibility that "the final result of the fatigue toxins is to produce a substance allied to the drugs with a stimulating action . . . " (p. 344). Some of the fatigue products might thus act to make the operation of the nervous system temporarily more efficient.

While these results and tentative interpretations are interesting and important two facts, at least, it seems render them unacceptable at their face value. In the first place only one subject was employed. Although the effects of fatigue are fairly consistent throughout her records, it is not at all improbable that these effects would differ for different subjects. If a group of subjects had been used, Smith's most striking finding, *i.e.*, the stimulating effect of insomnia, might have appeared only in certain individuals. At any rate, as she herself suggests, the experiments need repeating on a larger scale. In the second place, Smith, in interpreting her results, has emphasized physiological explanation to an extent which is hardly justified. It does not seem at all unlikely that the positive effects of insomnia were due to the subject's greater relative interest in the general experimental situation on the day or days immediately following the insomnia.

Patrick and Gilbert in their paper of 1896 (*Psychological Review*) noticed that even a moderate amount of sleep is capable of com-

pensating for a condition of unusual fatigue, but Smith's experiments indicate that "the time taken to return to a normal condition after the loss of but a few hours' sleep is disproportionately great; and this return is gradual but irregular" (p. 349).

Smith's subjective reports are in essential agreement with those of Patrick and Gilbert. There were disturbances varying from slightly increased irritability to actual hallucinations.

FASTING

The effects upon mental capacity of going without food are by no means clearly made out. This may be due to a number of causes. The effects may be very complex, the tests so far employed may be lacking in delicacy, and important after-effects have undoubtedly escaped notice due to the fact that adequate "after" tests on this condition have yet to be made.

Langfeld (50) made daily examinations of Agostino Levanzi, a lawyer of Malta, who underwent a 31-day fast at the Boston Nutrition Laboratory of the Carnegie Institution of Washington. Due to unavoidable circumstances there were only three days of testing before the beginning of the fast and no adequate tests at the completion of the fast. A considerable number of tests were used and from their results Langfeld concludes that: "In a word there was a loss in muscular strength due probably to loss of tissue, a possible gain in sensory acuity and a decided increase in the efficiency of all the central processes" (p. 48). (The efficiency of the central processes, of course, is supposedly measured by the more complex intellectual tests.) The loss in muscular strength is obviously a fact caused by fasting. The improvement in sensory acuity and "central processes" is not so clearly so. Possibly in certain of the sensory acuity tests and surely in the "central processes" tests there is a rise due to practice which is to be expected, fast or no fast. The only conclusion to be drawn, then, in respect to these tests is that practice effects in them were not obliterated by the period of fasting. Such effects may have been either positively or negatively accelerated, or they may have been unmodified. The subject's belief in the advantages of fasting was such that introspective reports could not be relied upon. There were some indications of irritability which may have been caused either by the fast or by the monotony of the subject's life under the conditions of the experiment.

In a paper entitled "Individual and Sex Differences Brought out by Fasting" (57) Marsh reports an experiment in which he

and his wife were subjects. During the first week feeding was gradually reduced from normal, during the second week no food was taken, and during the third week feeding was gradually increased to normal. A number of physical and simple and complex intelligence tests were given daily during the experiment. No control data (from "before" and "after" tests) are presented. The results indicate a positive correlation between food intake and vitality and strength. There is no clear-cut finding in regard to the intelligence tests. What so-called sex differences appear are naturally of no significance, because there is not the slightest guarantee that similar or greater differences would not under the same circumstances appear in the records of two subjects of the same sex. Subjective reports indicate pronounced emotional disturbances due to the fast. These varied from a condition of apathy to one of extreme excitability.

In examining such studies as those on fasting and loss of sleep one can not help but be struck by the fact that, whereas effects revealed in test scores are fairly meager, those revealed in introspective reports are often pronounced and definite, even if unsystematic. Indeed, it is not unlikely that further studies along these lines will be greatly aided if they contain in their programs some provisions for controlled (to the extent to which they can be controlled) introspective observations. Of course another line of procedure lies in the direction of more refined tests.

EFFECTS OF DRUGS

Rivers, as has been noted, emphasized the lack of adequate controls upon the subject's attitude in the earlier studies of work. Watson (p. 360) points out three additional limitations present in many of the experiments on the effects of drugs. (1) "only in rare cases has a function been practiced sufficiently in laboratory studies before control tests were introduced to gauge the effects of drugs, continuous exercise, loss of sleep and other factors." (2) "Too narrow functions have been studied—rarely has any of the work been done upon functions used in daily life. . . ." (3) "the functions have been followed through for entirely too short periods for wide generalizations to be made." In general, research workers have become increasingly careful to avoid these sources of error, but it is frequently impossible to avoid all of them. Take for example, the case of the subject's attitude. In some cases it is practicable so to disguise a given drug that the subject will not

know whether he has taken caffeine, say, or sugar of milk. Experiments with alcohol and tobacco, however, present more difficult problems. The flavor of alcohol is not easily disguised and, if doses of any size are employed, its almost immediate subjective consequences are quite apparent. And a really adequate experiment on tobacco smoking should provide for a control in the form of smoke from which all effective elements have been removed. It is likely, however, that where completely disguised doses of a given drug are impossible, longer series of measurements with more subjects eliminate to a considerable extent differences due merely to attitude.

Alcohol.—Experiments reported in 1911 or earlier (48) indicate that alcohol usually acts detrimentally on mental functions. This is quite certain where complex processes are involved and where the doses are moderate or large. A possible exception appears in a study by Langfeld (51). He found that alcohol reduced certain association and reproduction times without affecting the accuracy of these processes in any certain manner. While the results from very small doses or in very simple processes are doubtful there is at least no clear evidence here for either permanently or temporarily increased efficiency. It seems quite probable that where small doses failed to cause bad effects it was due either to the extreme variability of the function studied or to the fact that the subject put forth an increased effort to offset a lowering capacity of which he was aware. From these earlier experiments it also appears that the effects of alcohol are more evident in the quality than in the quantity of work.

Later work has on the whole substantiated the conclusion that alcohol has a detrimental effect upon mental operations. Frankfurter (30) working with himself as subject, found that 20 c.c. doses of alcohol decreased the accuracy of his typewriting and also, be it noted, his speed. While Lange and Sprecht (49) found that the visual and auditory stimulus thresholds were lowered by small amounts of alcohol, the difference thresholds for the same senses were raised.

Dodge and Benedict (25, 26) conclude from the results of an extremely careful series of experiments that alcohol is a depressant rather than a stimulant. This is shown in the following tabulation of their results quoted by Hollingworth and Poffenberger (40, p. 172):

	Per Cent.
Increase of latent time of knee-jerk.....	10
Decrease in the thickening of the quadriceps muscle.....	46
Protective eyelid reflex, latent time increased.....	7
Extent of eyelid movement decreased.....	19
Eye reactions, latent time increased.....	5
Speed of eye movements decreased.....	11
Sensitivity to electric stimulation decreased.....	14
Speed of finger movements (tapping) decreased.....	9

Miles (61, 62) has in two papers published findings which in general verify those of Dodge and Benedict. In spite of such results as those of Lange and Sprechert in regard to visual and auditory stimulus thresholds, we are safe in concluding from the thorough and rigorous experiments of Dodge and Benedict, and Miles, that even in the realm of comparatively simple processes alcohol is mainly a depressant and that the detrimental effects of alcohol (this conclusion finding support from Frankfurter) are clear in respect to speed as well as in respect to quality of work.

McDougall and Smith (60) obtained results demonstrating that alcohol usually has a deleterious effect upon mental efficiency. Varying doses of alcohol increased the number of errors in the dotting machine experiment as follows:

Dose	Increase in Errors, Per Cent.
10 c.c.....	21
15 c.c.....	42
20 c.c.....	39
25 c.c.....	113

A memory test also showed that alcohol lowers efficiency.

There is an exception to the generally deleterious effect of alcohol in the records of McDougall and Smith which is very interesting. It appears that when alcohol was taken at any time during a period of several days following a loss of sleep and while efficiency was dropping as a result of that loss of sleep, then alcohol still farther reduced efficiency. When, on the other hand, alcohol was taken during the period in which the subject's efficiency, lowered by loss of sleep, was gradually rising, then alcohol increased efficiency. These generalizations, if they can be substantiated, are obviously of first rate importance. That they need substantiation is evident from the fact that they seem to be based upon the performance of one subject.

The same investigators studied the effects of two different concentrations of alcohol. Twenty c.c. of absolute alcohol diluted

to 60 c.c. water increased the errors in the dotting machine test 160 per cent. 20 c.c. of absolute alcohol diluted to 200 c.c. of water, on the other hand, increased the errors by only 22 per cent. They also report that the effect of alcohol is much less evident if the alcohol is taken with a meal. When 30 c.c. was taken under such circumstances it had little or no influence upon efficiency.

The efficiency reducing capacity of alcohol is farther verified by Jörger (47) who found that this drug increases free association time and lowers the quality of the response and by Arlitt (4) who found that alcohol affects the maze-learning ability of white rats to which it is fed and also that of their offspring.

The old notion, still popular in some non-scientific quarters, that alcohol may have at least temporary beneficial effects probably grew out of a misinterpretation of a very real fact about alcohol, namely, that the subject who has taken a moderate amount of it is inclined for a time to over-rate the excellence of his own performance. McDougall and Smith report this tendency and popularly we are aware that many a story of keen wit becomes dull when its audience is free from the effects of alcohol.

The possibility remains that there are important individual differences in the degree of susceptibility to effects of alcohol. In fact the reality of such differences hardly needs confirmation. But whether some individuals are actually stimulated by a drug which depresses others is still another matter. Dunlap (27) tested certain eye-hand coördinations of two subjects each of whom were given within about one hour 7 oz. of rye whiskey. One of these subjects became obviously drunk and his coördinations clearly disturbed. The coördinations of the other subject seemed to improve a little during the experiment, and he felt no ill effects until a vomiting spell 8 hours after taking the whiskey. The data are too meagre, however, to justify the conclusion that the second subject was made even temporarily more efficient by the whiskey.

Caffeine.—Most of the evidence which we have upon this subject indicates that moderate amounts of caffeine (amounts such as are found in the ordinary cup of tea or coffee) increase the efficiency of mental activity, and there is little reason to believe that this rise in efficiency is followed by a measurable decrement. The effects of larger doses are sometimes unfavorable, depending somewhat upon the operations considered and the subjects.

Hollingworth (38) has presented the more important experimental facts reported prior to 1912. The following table gives the main results of his own investigations;

St. = stimulation

o = no effect

Ret. = retardation

Test	Small Doses	Medium Doses	Large Doses	Secondary Reaction
1. Tapping.....	St.	St.	St.	None
2. Three-hole.....	St.	o	Ret.	None
3. Typewriting				
(a) Speed.....	St.	o	Ret.	None
(b) Errors.....	Fewer for all doses			None
4. Color naming.....	St.	St.	St.	None
5. Opposites.....	St.	St.	St.	None
6. Calculation.....	St.	St.	St.	None
7. Discrimination reaction time.....	Ret.	o	St.	None
8. Cancellation.....	Ret.	?	St.	None
9. S-W illusion.....	o	o	o	
10. Steadiness.....	?	Unsteadiness		None

The sleep of the subjects was generally untroubled except where large doses were taken. In that event such symptoms as headache, dizziness, feverishness and irritability were also liable to be present. The degree to which sleep and feelings of well-being were affected by caffeine varied with the body-weight of the subjects. The heavier ones were less affected.

Briefer experiments by Langfeld (51) and Tolman (89) support Hollingworth's findings. According to the former caffeine decreases association time, and according to the latter small doses of caffeine (1.5 grains) increase one's ability to memorize and, at the same time, lower one's susceptibility to retroactive inhibition. In opposition to the general trend of these results we have those given by Schilling (77). He found that simple reactions to an auditory stimulus are, if anything, retarded by 5 grain doses of caffeine. It should be noted, however, that such doses are relatively large. Lashley (52) reports that caffeine lowers the learning capacity of white rats.

Hollingworth points out (p. 166) that quantitative results from investigations of the effects of caffeine in its pure form cannot be compared uncritically with its effects when administered in tea, coffee and other beverages. Hollingworth's experiments show that caffeine has a greater effect when taken on an empty stomach. This statement may well be kept in mind in regard to other drugs. The effects of alcohol (see McDougall and Smith) for instance, vary considerably according to the form of administration.

Tobacco.—It has been established for some time that excessive smoking is injurious to the physical organism. Just what amount of

smoking should be thought of as excessive and just how injurious a given excess is depends within rather wide limits upon the individual smoker. Certain correlations have also been pointed out between tobacco smoking and such measures of intelligence as school marks (12). With practically no exceptions groups of non-smoking students make better marks than groups of smokers. But the earlier disposition to interpret this fact as a sign that tobacco smoking has a measurably detrimental effect upon intelligence has been checked by more careful analysis. It is quite possible that where small boys take up smoking they do so because they lack certain social inhibitions, and the lack of these same or closely related inhibitions may also be a cause of their disregarding scholastic duties. Among older boys perhaps the socially more intelligent are in a position where the tobacco habit is more easily cultivated. And they may also be in a position where there are a maximum number of distractions from purely scholastic activities. Thus one aspect of intelligence might be the cause of both tobacco smoking and low scholarship. In other words the true state of affairs is far more complex than that suggested by the statement of a single causal relationship between tobacco smoking and intelligence or even tobacco smoking and scholarship.

Experimental studies of the effects of tobacco smoking upon mental functions indicate in general that, whatever the amount of this effect, it is detrimental. While Berry (11) found that he added slightly more accurately or more rapidly after smoking a cigar, Bush (14), Johnson (45) and Froeberg (31) obtained some evidence of an advantage in the other direction. As far as these experimental studies go they indicate that the effects of tobacco smoking on mental efficiency are very slight. Before taking such a conclusion seriously, however, one should examine some of the limitations of its factual support:

1. During these experiments the smokers used much less tobacco per day than does the ordinary user of the drug.
2. The effects considered are those accruing from small and isolated amounts of tobacco smoking rather than from the relatively continuous indulgence characteristic of smoking in everyday life.
3. Although Froeberg had some slight success along this line, no one has yet devised an adequate control dose (*i.e.*, a dose minus any effective agents) of tobacco smoke. Consequently differences of attitude are bound to arise between the "smoke" and "non-

smoke" tests or groups for which it is impossible to make accurate allowance. Such attitude differences are especially important where the data do not point very strongly to the advantage of one condition, as in the case of these experiments in tobacco smoking.

So far it has not been proved that tobacco smoking aids mental efficiency. But for moderate smoking neither has the opposite been clearly established. Experimental interest in the subject has probably been too general in nature. A comparison of the effects of tobacco with those of no tobacco is an indefinite procedure. Better a measurement of the effects of doses of tobacco varying in size or in frequency. Studies might well be made of the development of tobacco tolerance (a fact which most smokers know to be quite real) as measured by mental efficiency, and also of the loss of that tolerance.

Writers who wish to defend tobacco smoking or to seem non-partisan to tobacco smokers sometimes argue that, whereas tobacco smoking may have some detrimental mental effects, so has the elimination of tobacco smoking a detrimental influence upon the smoker who is not accustomed to doing without it. This of course, holds good for any drug, and it is probably less true for tobacco than it is for the more harmful drugs, such as alcohol, opium, etc.

Strychnine.—Poffenberger (72) has summarized the effects of strychnine as measured up to 1914. Small doses of this drug are said to improve the appetite and to give the subject feelings of strength and optimism. Ergographic work is first positively and then negatively accelerated by strychnine.

Lashley (52, 53) reports that proper injections of strychnine increase the maze learning ability of rats as measured by the amount of practice required to achieve complete learning. Retention is apparently unaffected. In the experiments of McDougall and Smith (60) one subject was given doses of from $1/15$ to $3/15$ grains of strychnine at intervals. The effects were marked. The dotting machine test showed a decrease in errors of 15 per cent. and memory for lists of associated words was similarly improved. Strychnine also increased the number of alternations in a reversible perspective figure.

Poffenberger's own findings are mainly negative. Ordinary therapeutic doses ($1/30$ to $1/20$ grain) interfered slightly with steadiness, but tapping, aiming, and a wide range of intellectual activities were unaffected. He explains his results as perhaps due to the alleged fact that strychnine acts upon the cord and medulla to a

greater extent than upon the higher centers. This suggestion does not touch the lack of harmony between Poffenberger's results and those of other investigators, however, and Watson (p. 368) holds that Poffenberger may not have employed suitable doses for the particular subjects tested. This is at least plausible when we note that McDougall and Smith who did obtain positive results used 1/15 grain as their smallest dose, whereas Poffenberger's largest dose was 1/20 grain.

Other Drugs.—Results obtained by McDougall and Smith indicate that the maximal effect from 40 to 90 minims of tincture of opium appear between 75 and 115 minutes after taking. The first effect as shown by the dotting machine test and by a memory test is one of increased efficiency lasting from 30 to 40 minutes and followed by a drop to a level considerably below normal. Alterations of reversible perspective which are decreased by alcohol and chloroform are increased by opium just as they are by strychnine. The amount of stimulation supplied by opium varies apparently with the fatigue state of the individual. When a subject who had suffered from loss of sleep was on a plane of efficiency below her normal one the stimulating effect of opium was most marked. When the subject was under the influence of opium the test task seemed relatively easy somewhat as it had in the case of alcohol. But where opium was the drug, objective efficiency corresponded more nearly to subjective estimates.

Macht and Isaacs (55) report that doses varying from 1/16 to 1/4 grain of morphine act as stimulants, decreasing reaction time and increasing accuracy of response. The initial period of stimulation is shortest with the largest doses of the drug. Following this is a period of depression during which efficiency is below normal. Effects essentially unlike those of morphine are reported by Macht, Isaacs and Greenberg (56) for such drugs as quinine, aspirin, phenacetin and salol. Reaction times here were lengthened; there was no period of stimulation.

One may well conclude from all these researches that in the future the action of few drugs will be considered understood until their effects on mental activities have been investigated.

COLOR AND BRIGHTNESS OF SURROUNDINGS

While there is, or has been, a good deal of speculation about the effects of color and brightness upon human working capacity, there is little in the existing literature (aside of course from that on

the purely optical aspects of the situation) to warrant definite generalization. Pressey (74) reports that his experiments show some slight evidence for a speeding up of certain functions (tapping, multiplication, and continuous reaction) with increasing brightness of the subject's surroundings. There is no evidence for a potency of hue independent of brightness. Similarly Johnson (42) found that subjects sort cards by touch better when working in a good light than when working in darkness, even when visual aids to sorting are lacking in both cases. The mere presence of light as a stimulus may so increase general muscular tonicity that the organism's capacity for work is increased. Burt (13) made tests of attention and reaction time on subjects in evenly and unevenly illuminated sections of a city street and on subjects working under analogous conditions in the laboratory. His results show greater efficiency for the conditions of non-uniform illumination.

ATMOSPHERIC CONDITIONS

Psychological investigations have been made of the effects of the CO₂ content of the air, of temperature and of humidity. Bass (7, 8), and Thorndike, Ruger and McCall (86) report that within the limits of their experiments, ventilation by recirculation instead of by introducing outside air has no noticeable deleterious effect upon the intellectual processes. Thorndike and others (87) compared the psychological effects of ventilation conditions varying in regard to temperature, humidity, admission of outside air, and circulation of the air. No differences in efficiency were noted between work done under supposedly favorable conditions (68° F., 50 per cent. rel. hum., etc.) and that done under supposedly unfavorable conditions (86° F., 80 per cent. rel. hum., etc.) Stecher (80) studied the effects of humidity, temperature being kept constant at 75° F. Two humidity conditions were compared; (a) relative humidity of 50 per cent. and (b) relative humidity of 20 per cent. Both subjective reports and the scores from ten tests fail to show any significant effects. "Psychology," Miss Stecher concludes (p. 85), "has given its answer, so far as it can in the present state of the science, to the question as to whether the ventilation conditions commonly found have any effect on the practical activities of life. If this absence of demonstrable effect is due to a constant adjustment of the organism that will eventually result in strain, it is for physiology to trace any subtle, long-time ill-effects that may have escaped the behavior tests."

The psychological effects of oxygen deprivation were very carefully studied during the war (1, 9, 43, 44, 92) in connection with the aviation service. Lack of oxygen was secured by a re-breathing apparatus by means of which the subject rebreathes the air from a supply tank, thus reducing the oxygen content, or by means of a diluting apparatus. These devices were standardized with reference to a low-pressure tank in which air can be rarified to correspond to any given altitude. In the British investigations (21) the effects of oxygen deprivation were frequently determined by tests after flights. Ability to stand oxygen deprivation varies considerably. Of each 100 carefully selected American flyers only 61 are physically and mentally capable of attaining an altitude of over 20,000 feet with safety; 25 out of each 100 are physically and mentally unsafe at altitudes above 15,000 feet; and 14 out of each 100 are physically and mentally unsafe at altitudes above 8,000 feet. Of psychological processes the more highly organized are impaired first. Oxygen hunger has accumulative as well as immediate effects. One hundred hours of flying at high altitudes may render a man at first capable of effective work at 20,000 feet, incapable of effective work at 10,000 feet. On the other hand a subject may become adapted to a limited degree of oxygen deprivation.

The qualitative changes in behavior have also been reported in some detail. Depletion of the oxygen supply is accompanied by muscular twitching and tremor, incoördination, and over discharge. The subject becomes less and less capable of normal attention, until finally he will heed instructions only when they are given insistently and in a loud voice. In the final stages emotional disturbances are not uncommon. Some subjects express uncontrolled anger, some become sullen, and others become silly.

SEASONAL VARIATIONS

Peaks (70) has reported a rather elaborate historical and experimental study of seasonal variations in efficiency. His own experiments, like those of earlier investigators, show that there are seasonal variations in mental efficiency. In this case there was a depression in mental ability which appeared in the winter shortly after the appearance of a more noticeable and longer lasting depression in physical capacity. The interpretation of this fact is, however, almost impossible. Peaks seems to think that the larger yearly variations in abilities are due to the combined influence of temperature and light strength. But he admits that the following

factors may have some influence: food, clothing, body temperature, humidity, air pressure, practice effects, previous mental and physical development, health, etc. If this is true, correlations between efficiency and some of the simpler and more definable conditions of life are probably more worth noting than correlations between efficiency and such vague conditions as are covered by the names of the months and the seasons.

DIURNAL VARIATIONS

Since Marsh in 1906 published his study of the diurnal course of efficiency a number of attempts have been made to test and extend his findings. These investigations have usually been concerned with diurnal variations either as indicators of the amount of fatigue induced by the work of school or factory, or as indicators of variations in efficiency. Heck (37) and Martyn (58, 59), for example, seem to have been interested mainly in the effect of the day's work upon school children, while Winch (93, 94, 95) and Gates (35, 36) came at the matter more with the purpose of determining the most efficient hours of the school child's day. These differences in point of view, however, are not, in recent writers at least, accompanied by a narrowness in the interpretation of the facts of diurnal variation. Those who are interested in such variations as indicators of fatigue would hardly deny the complexity of a valid conception of mental fatigue, and those who are interested in these variations as indicators of an organic periodicity would certainly admit their dependence upon the type of occupations indulged in during the day.

From the facts now available it is impossible to arrive at any statement of the diurnal course of efficiency which has a very wide applicability. According to Gates, Peaks, and Robinson (76) mental efficiency of school children rises during the morning, shows a low point early in the afternoon and after that another rise which probably is followed by another low point. Such findings are not to be criticized as facts and as facts they probably may be used to advantage under certain peculiar conditions. Nevertheless they can not be considered as broadly typical of diurnal fluctuations. Hollingworth (39) found a falling off in mental efficiency during the day and so did Arai (3). Muscio (65) as has been reported, found that feelings of fatigue increase throughout the day except for an early afternoon recuperation. One can explain these differences in terms of the subjects employed, the nature of the day's work, changes

in temperature and other atmospheric conditions, meals and the like. If this is true, it seems reasonable to believe that, insofar as there are differences in the efficiency of work at different hours, these differences are themselves to be explained in terms of the subjects, the work, etc., rather than in terms of the characteristic natures of different times of day. Each time of day, like each time of year, covers such a host of psychological possibilities that it seems futile to work out its correlates except for some extremely practical and immediate purpose. It may, of course, be well worth while for employers and educators to know the specific diurnal variations of specific groups of employees or students working under conditions which for them are standard.

INDIVIDUAL AND GROUP DIFFERENCES

The literature on mental tests contains a wealth of material regarding differences in mental efficiency between individuals and groups of individuals, a review of which would hardly be appropriate in this place. Investigators concerned with topics usually included under *mental work* have not as a rule paid much attention to individual or group differences except insofar as these differences have had a bearing on the validity of experimental results. Once in a while an exception has appeared. For instance, Winch (94) presents a relationship between the nature of diurnal variations and age; and Garth (33) presents a possible relationship between fatigability and race. In another study (34) Garth attacks the claim made by Meumann, Kraepelin, and others that work curves in computation experiments fall into types. Garth admits that the curves collected by him vary within wide limits as to general trend, but none save the average curve is modal.

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RECENT LITERATURE ON THE PSYCHOLOGY OF THE MUSICIAN

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The most significant contributions to the psychology of the musical mind as it functions in the musically talented person, or artist, have appeared since the year 1915. Prior to that year there was an abundance of psychological literature concerning the nature of the material of music, from the single tone to the complete melody, but the creator and reproducer of music received only a passing interest and an occasional expression of opinion from psychologists. Stumpf (17), for instance came in contact with a Spanish boy prodigy and disposed of him in a two-page paper!

The musician began to come into his own in experimental psychology with Rupp's (14) contribution to the psychology of talent in the form of a prospectus for future research. In 1913, Revesz (11) made a preliminary report on an analysis of a boy especially gifted musically, but it was not until 1916 (12) that the full report appeared, and not until 1920 (13) that he published an inventory of musicalness based on his study of the boy prodigy. The problems with which Revesz concerns himself in the 1920 study are: 1. Can a method be devised which will identify the musical mind before the beginning of instruction, even in cases less exceptional than that of the boy examined; and, 2. Could such a method be applied to adults and to the musically trained? His aim, then, was to find a method of identifying the musical, adapted to children and adults, and to the musically trained and untrained. For this purpose he devised eight tests, based upon musical and acoustical factors, and gave these to 63 boys and girls from seven to twelve years of age. The tests are as follows: 1. *The rhythmic sense*. Twelve rhythmic groups were arranged in order of difficulty. In the first part of the test the rhythms were presented monotonically and the children were asked to reproduce the rhythm with hand-claps. In the second part of the test the same rhythmic forms were presented melodically and a like response was called for from the children. 2. *Absolute pitch*. For the second test the experi-

menter sounded a tone on the piano and the observers were called upon to reproduce the tone on the instrument. 3. *Octave recognition and transposition*. This test Revesz found too easy for his observers. 4. *Relative pitch*. He first produced an interval and then sounded any tone on the piano and asked the child to reproduce vocally the given interval, with the new tone for its basis. 5 and 6. *The harmonic sense*. In these tests the children were asked to reproduce vocally the constituent tones of chords. 7 and 8. *Melodic memory and playing by ear*. The seventh test deals with memory for melody, and the eighth with the ability to play by ear a known tune.

Revesz assumed the vocal reproduction of a melody to have the greatest diagnostic value, and by correlating the other tests with the seventh test, he concluded that the sense of rhythm does not possess great musical significance, but that the spontaneous instrumental reproduction of a known melody, absolute pitch, vocal transposition of an interval, and also the ability to analyze simultaneous intervals, are characteristic manifestations of musicalness. Revesz's investigation has two great merits—his inventory has an empirical basis, and his test material has musical significance. The number of his observers was too small, however, for the establishment of norms.

In his study on the earliest appearance of talent (11), Revesz claims that musical talent necessarily makes its appearance in early youth; at first appears technical equipment, or virtuosity, later, power of interpretation, and then creative power. An examination of the great composers shows that the full development of the creative powers is reached between the twentieth and thirtieth years.

In contrast to Revesz, Seashore's inventory (15) has a theoretical basis, while the main factors of his inventory are acoustical rather than musical. One is entirely justified in offering the criticism that Seashore measures individual differences in acoustical sensory capacities, rather than differences in musical powers. The Seashore tests are rapidly gaining admittance into the class-room and studio as a basis for vocational guidance in music and for this reason, if for no other, they should be subjected to a rigid and thorough examination. Seashore's work is epoch-making in that he is the first to devise rigid scientific tests for measurement, the first to establish norms for musical talent, and the first to make the tests available for the music teacher in studio and class-room. No further mention of this first treatise on musical talent is necessary as it is readily available to readers in English.

Several briefer studies are available that help to throw light upon specific factors in the more extensive treatments of Revesz and Seashore. Schüssler (18) attempts to account for the unmusical child in a paper based upon historical and statistical material. In the first part of the paper, he reviews briefly the works of Billroth, Max Meyer, Stumpf, Von Meyer, Rupp, Revesz, and several others, as well as the contributions on the anatomical and physiological bases of musicalness. His own contribution consists of a statistical examination of two hundred unmusical children, the criterion for unmusicalness being attainment of a certain grade in school singing. Schüssler's conclusions, based upon the historical review and his statistical data, are as follow: 1. There are between five and ten per cent. of unmusical people. The majority of the unmusical, if not all, are not beyond profiting by musical instruction, and should therefore not be neglected, but rather given well-planned and intensive instruction. 2. For musical or unmusical capacity, interval memory is of greatest significance. 3. Absolute pitch is of significance only for outstanding musical gift. The unmusical, as compared with the musical, are as a whole less gifted than the musical, *i.e.*, they are not only inferior musically, but also in other mental capacities. 4. Forty-one per cent. of the unmusical, fifty-seven per cent. of the half-musical, and seventy-nine per cent. of the musical, make satisfactory progress in school. The school work of the musical is fifteen per cent. superior to that of the unmusical.

Schüssler's findings on the relation between intelligence and musicalness are in accord with those of Stumpf, who found that musical children are, as a whole, better students than the unmusical.

The Pannenburgs (10) examined 3,860 children, 423 musicians, and the biographies of twenty-one composers in an attempt to derive the intellectual and emotional characteristics of the musician. Their conclusions agree with those of Schüssler and Stumpf. Of all the children, 342 boys and 152 girls were designated as musical. The musical children were advanced in their school work beyond their years, while the musician was found to have extensive intellectual interests and to be widely gifted, particularly in languages, literature, history, geography, mathematics, natural history, and drawing.

Bernfeld (1) claims that "the individual's reaction to music is by no means wholly decided by the nature and quantity of his psychophysical tendencies. It is influenced to a certain degree by the will to be or not to be musical. Even when accurate tests

have shown that a person possesses all the elements of musical ability, it cannot be foretold with certainty whether he can acquire musical appreciation, for it is possible that an inhibition with retro-active force against music may exist in him, a will to be unmusical, or at least to seem so to himself and others."

Kovacs (6) contributes a much needed study on musical memory. His own experience as a piano teacher taught him that his pupils, although very musical, were incapable of playing through a musical composition from memory with any degree of certainty, in spite of great effort. In planning his experiment, the author concluded that the visual factor did not play an important rôle in playing from memory, since the pupils could write the composition from memory and could also easily memorize poetry, numbers, etc. Neither could he ascribe the difficulty to motor adjustments, because of the technical proficiency of the pupils, and because the memory failures did not occur at the most difficult passages. The pupils had studied the piano at least eight years, so that their deficiency could not be due to lack of visual and motor coördination. He therefore concluded that the difficulty must be of an acoustical nature. In other words, the pupils could not hear, inwardly, that which they were playing. He asked the performers to play the melody of a composition with which they were thoroughly familiar an octave above and found that they were incapable of so doing. Neither could they reproduce vocally the alto or bass voices of a memorized fugue. With these problems before him, he initiated an investigation to answer the following question: How is a musical composition most readily committed to memory, by practice at the piano, or away from the piano? His observers were all accomplished pianists and piano teachers. He concludes that the difficulty that music students experience in memorizing musical compositions is due to the lack of a thoroughly trained "inner" and "outer" tonal hearing. By "outer" hearing he means the ability to name tones heard, and by "inner" hearing, the ability to image tones from the staff. The student should begin his piano study, according to this writer, with note-reading.

In another study (7), Kovacs deals with the relation between recognition and reproduction as they function in musical memory. Is recognition necessarily involved in reproduction or are the two independent factors or drifts of memory? Three differences were found to exist between the two types of memory: 1. The attitude in recognition is more passive, in reproduction, more active; 2. In

recognition attention is fixed more on the memory material as a whole, in reproduction on the sequences of the constituent sections; 3. The imagery used in recognition is often different from that used in reproduction. The author concludes that he who possesses good recognition, but poor reproduction, may become a good listener, but not a performer. The artist must possess both, while the teacher stands more in need of recognition, and the virtuoso of reproduction. The student, in preparing for public performance, should be careful not to confuse the two types of memory in his practice. Reproduction must predominate over mere recognition.

Heinitz's study (4) on musical reproduction is most valuable in that he suggest, in a preliminary way, ways and means to determine native adaptibility for musical performance on different instruments. The instruments used by the author were the piano, violin, and flute.

Gatewood's research (3) on finger reactions is suggestive in its bearing on musical performance, and therefore valuable for vocational guidance in music, in that she finds that there are some individuals who, in spite of unlimited practice, will not attain the speed and accuracy which some others show at the initial trial. Morton (9) reports the results of discontinued experiments on the degree of accuracy attained by pianists in playing double against triple time.

Copp (2) argues that although musical ability is innate and inherited, "almost everyone possesses the heredity . . . although great genii will doubtless continue to be sporadic and unaccountable, real musical ability is much more common than has been supposed . . . the very large majority of children lose their native musical ability through lack of training of the ear and mind during their most susceptible period." Copp bases this sweeping statement on the fact that eighty per cent. of normal children can be trained, according to her experience, to sing "Center C" on demand, and to recognize it when it is played or sung. "This surely indicates that musical talent is much more widespread than has been thought."

The study of the nature of the musical response still follows in the main the questionnaire, introspective, and purely speculative methods. But one brief recent study of the physiological effects of music is available, and that one is incomplete. Hyde and Scalapino (5) experimented with Tschaikowsky's Death Symphony (which symphony is that?), the Toreador song from Carmen, and a Sousa march, in order to "ascertain the effect of different kinds of music upon the heart and blood pressure in individuals who are

known to have musical talent and are fond of music; also persons who are indifferent and have no fondness for music, in neurasthenics and in some animals." They find that the minor tones of the symphony increase the pulse rate and action current of the ventricular contraction, and lower the systolic and diastolic pressures. The Carmen song and the march increased the systolic and pulse pressure, but the former also increased the pulse rate, with decreased diastolic pressure and action current, while the march slowed the cardiac cycle and increased its action currents. The authors do not state whether these results were obtained with the musical or unmusical subjects.

Washburn (19) seeks to answer the questions: 1. How far does musical enjoyment or pleasure depend on principles not governing our other enjoyments? 2. How far and why is the relation of music to emotion unique? For the first question the sources of pleasure derived from single tones and tonal combinations, simultaneous and in sequence, are considered. The author concludes that all sources of musical pleasure are sources not peculiar to music. We enjoy music because we enjoy other things. The uniqueness of music in relation to the emotions is that it reaches these directly, without reference to associations based upon the individual's experience. "Musical sound is the kind of sound that is produced under stress of emotion; rhythmic sound means that the emotion is magnified and exalted by being shared with the whole mass of one's fellow beings."

To Lee (8) the question of what music *is* is synonymous with what music *does* to the mind of the hearer, or "more correctly, of what the mind of the hearer does in response to the music which he hears." With this as a guiding principle, the writer carried out an experiment in order to determine the varieties of musical experience. The subjects were asked in a questionnaire to react to the question: When music interests you at all, has it for you a *meaning* which seems beyond itself a *message*; or does it remain just music? The questionnaire contained also sets of queries to determine the degree of musical endowment of the subjects. Half of the subjects answered that to them music imparted a message, a number of these reporting that they were attracted to music primarily because of the images, emotions, and trains of thought which it suggested. The other half contended that whereas they found a meaning in music, that meaning was inseparable from the music itself, and that visual images or emotional suggestions were either excluded

or insignificant. The subjects who repudiated the message of music, to whom music had a meaning in itself, were found to be those who were most musical.

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THE PSYCHOLOGY OF LANGUAGE

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The present review deals with the contributions to the psychology of language which have appeared in periodical literature during the five-year period, 1917-1921. Such a review, incomplete though it may be, is yet sufficient to indicate that this field is not being extensively worked by psychologists, and this in spite of the increasing recognition of the fact that a large proportion of the apparently most perplexing problems in psychology are really language problems. Indeed, it seems likely that many of the problems of the other sciences will find their solution in an investigation of the genesis of the verbal reactions involved in the statement of these problems. The linguists and the philologists, too, have long been awaiting an adequate psychology for the interpretation of their data; those who have found the traditional psychological interpretations no longer satisfactory have in some cases undertaken to work out for themselves theories as to the nature of language behavior. From all of these points of view it seems highly important that language receive more attention from those whose proper business it is to investigate the fundamental principles underlying human behavior. Indications are not lacking that the next few years will witness an increasing amount of work by psychologists in this field.

The problem of the nature of language has received most attention from those engaged in the study of speech disturbances. Barat (1) summarizes the leading theories which have been thus evolved. He regards language as dependent upon the development of a system of automatic associations which function in the co-ordination of speech movements, in the analysis and interpretation of sensation, and in the evocation and identification of images. These associations cannot develop without a parallel differentiation of certain regions of the cerebral cortex, whose integrity therefore becomes necessary to the automatism of speech.

Sechehaye (17) has written an exposition and interpretation of the linguistic doctrines of the philologist Ferdinand de Saussure.

According to these doctrines, we must distinguish between the speech acts of individuals (*la parole*) and the system of conventions existing in the speech habits of a community (*la langue*). The latter is a system of arbitrary signs whose values are not inherent but wholly conventional. The fundamental principle is that there be as many distinct signs as there are distinct ideas. Ideas, however, do not precede their signs, since a community, not possessing a joint mind, can never seize an idea before assigning to it a sign. Both signs (*i.e.*, significant sound sequences) and the ideas which they represent have their individual values by virtue of the fact that each is *distinctive* as compared with the others. There is thus at any given time an equilibrium between all the elements of language. Because of its arbitrary, conventional character, language is not influenced in its organization by the mind, but is very susceptible to the influences of other factors, such as phonetic change. When influences of the latter sort disturb the equilibrium of elements, a new equilibrium is brought about by *interpretation*, *i.e.*, a disturbance of one element is made good by the adaptation of others. Thus, "language is at any moment a fortuitous resultant of this arbitrary combination of the sounds and the ideas which constitute it."

Schiefferdecker (15) has thrown some interesting light on the problem of the origin of speech. His investigation of the phylogenetic evolution of the muscles of mastication shows a marked differentiation in man as compared with animals, a differentiation due probably to the functioning of these muscles in speech. There is no gradual evolution through the primates to man, and in man himself the differentiation begins relatively late in the embryonic period and is not complete until the twelfth year of life. This indicates a late origin of speech in man's racial history, an origin which was contingent upon an adequate development of the brain. Of the three muscles investigated (masseter, pterygoideus internus, temporalis), the masseter showed by far the greatest differentiation, and would hence appear to be especially important in speech.

Thumb and Marbe in 1901 reported the results of an investigation of the psychological basis of analogical changes in language. From these results they concluded that the effectiveness of a word-association in bringing about analogical changes increases with the number of members of a speech community in whom it is present and with a decrease in the average time required for the association to take place. Esper (6) repeated this investigation with 126

American subjects (100 educated adults, 11 children, 15 uneducated adults). The results were similar to those of the German investigation; in both cases it was found that the greater the number of subjects who respond with an identical word, the smaller is the average association time for this response; in both languages, words of a given grammatical category were found to be associated predominantly with words of the same category; numerals from one to ten were associated predominantly with the next higher numeral; and adjectives with adjectives of opposed meaning.

Barnils (2) points out that heredity in language constitutes a fundamental problem still without solution. He believes that the cases reported of individuals producing speech sounds which have not occurred in their environment but which were characteristic of their ancestors call for a greater emphasis on heredity than is generally conceded.

In the field of phonetics, Jones (7) has shown the usefulness of the phonetic kymograph in analyzing speech sounds as to the presence or absence of voice and nasality, in measuring the length of sounds, and in determining the pitch of the voice. Scripture (16) has reported a case of speech without use of the larynx. A girl who had undergone tracheotomy at the age of three and was unable to make any air pass through the larynx could nevertheless speak distinctly in a faint almost toneless voice. Phonautograph records showed the efforts at intake of air as a preliminary to speech, but thereafter fairly regular waves comparable to those produced by the vibrations of the vocal cords in normal subjects. Observation revealed that in this case the subject produced "voiced" sounds by compressing the air in the pharynx and then allowing it to escape between the tongue and the velum, the surfaces of which were set in vibration and thus functioned as an artificial glottis. Peters (14) has investigated the influence of Sievers' "visual signals" and hand gestures on speech melody. Eight observers read a strophe of poetry four times, each time with a different combination of visual signals and hand movements. Both phonograph and laryngeal records were taken, the latter being then analyzed for the pitch relations. Hand movements in a plane parallel to the body surface were accompanied by relatively small variations in pitch; those in the direction of vision were accompanied by greater variations in pitch. Gestures toward the right and upward were accompanied by rising tone; those toward the left and downward by falling tone. With the use of the "Dur" signal smoother melodies occurred; with the use of the "Moll" signal, more broken melodies.

A large number of studies of the language development of children have appeared in the last five volumes of the *Pedagogical Seminary*. Magni (9) points out that actual recording of the vocabularies of individuals has shown that the earlier estimates of such vocabularies were far too low. Thus it was estimated that persons of fair intelligence have a vocabulary of about 3,000 or 4,000 words, while actual counting has demonstrated that children in the second grade have an average vocabulary of 4,480, and that this increases to 12,000 in the eighth grade and 20,120 in college. The vocabularies of children show great individual differences. The results of investigation indicate an average vocabulary of 400 or 500 words for a child four years old, although for some children the figure is much larger. Up to the sixth year, girls seem to surpass boys in vocabulary, but thereafter the boys seem to excel the girls. Nice (11) reports the speech development of a girl to the sixth year. She declares that "E. went through no 'parrot' stage, never repeating words after her parents. She never merely imitated words but used them when she needed them." The child's speech began at 14 months with three words; at 17 months she had a vocabulary of 46 words; at 18 months, 133; at 3 years, 1,139; at 4 years, 1,765; at 5 years, 2,502; at 6 years, 3,075. There was a progressive decrease in the proportion of words relating to personal experiences and indoor environment, and a progressive increase in those having to do with outdoor environment and abstract terms. The first sentence was heard at the end of the eighteenth month, but until after the second year the sentences lacked connectives, such as conjunctions, prepositions, articles, and the verb *to be*. Oatman (13) reports a boy of 18 months with a vocabulary of over 100 words. His first sentence was heard early in the eighteenth month. Brandenburg (4) has recorded the conversation of a girl for an entire day at 40 months and at 52 months. At 40 months the child used during one day 34 per cent. of her total vocabulary of 2,500 words, speaking a total of 11,623 words in 1,873 sentences, an average of 6.6 words per sentence. At 52 months she used during one day 24 per cent. of her total vocabulary of 4,200 words, speaking a total of 14,930 words in 1,967 sentences, an average of 7.5 words per sentence. Of the different words used, nouns were most numerous (42.3 per cent.) and verbs next (30.7 per cent.). Nice (12) recorded the conversation of a four-year-old girl for one day. The child used 54.5 per cent. of her total vocabulary of 1,135 words, speaking a total of 10,511 words in 2,686 sentences, an

average of 3.9 words per sentence. Of the *different* words used, the proportions of the various grammatical categories were about the same as those found by Brandenburg, but for the *total* words used the proportions were different, 28.2 per cent. being verbs, 22.1 per cent. pronouns, and 19.5 per cent. nouns. Bateman (3) gives statistics from his own investigations and those of others as to the time of advent of the first spoken word of the child. The data pertain to 35 children, 18 of whom are English-speaking, 12 German, and 5 French, Belgian, or Polish. The ages of the children at the time of the first word vary from 8 to 15 months. Of the 35 children, 15 or 42.85 per cent. spoke their first word in the period from 10 to 11 months, while 26 or 74.28 per cent. had begun articulate speech by the end of the first year. The boys in general seemed to begin later than the girls. The first words are with few exceptions nouns or interjections.

The languages of primitive peoples have received considerable attention. Magni (9) points out that their vocabularies have proved much larger than was formerly supposed. Thus the vocabularies of various American Indian languages have been found to contain from 10,000 to 40,000 words. Dornan (5) has investigated the language of the Bushmen of the Tati district of South Africa. Their language, called Sesarwa, contains three tones, by which words of similar sound but different meanings are distinguished. The list of sounds includes four "clicks." The author collected a vocabulary of 2,000 words, but does not believe that this is complete. Morphologic relations are shown mostly by suffixes and infixes. The noun has two numbers, but no genders or cases. Abstract words are rare. The numerals are from one to five. Verb forms are well developed; there are two voices, five moods, and a large number of tenses, with special forms for the negatives of these tenses. There is very little syntax. Kroeber (8) gives an account of the speech of a Zuni boy in his 23d and 24th months. The first week of observation revealed a vocabulary of about 18 words, six of which were nouns denoting consanguinity, and one a verb. At this time the child had not yet learned to articulate a number of Zuni sounds, such as the palatal stops, the affricatives *ts* and *tc*, the fricatives *s* and *c*, surd *L*, and a series of glottalized sounds. "So far as they occur, these are precisely the sounds with which English-speaking children have difficulty." Near the beginning of the child's 24th month, the observer heard the first "sentence," a noun and verb used together. During the six weeks preceding

his second birthday, the child increased his vocabulary by half or more and learned to articulate a forward palatal stop and an alveolar affricative, but did not acquire the fricatives *s* or *c*, surd *L*, nor any glottalized consonant. "He had, however, mastered a characteristic tonal inflection, was beginning to unvoice according to rule, and was drifting away from his early leaning to make every word consist of two identical but separate syllables. Of grammatical structure there was as yet no trace, and sentence building remained sporadic and of the most elementary kind." Michelson (10) discusses the method of approach to the problems presented by the multiplicity of linguistic stocks on the American continent. Chief among these problems is the question of whether this multiplicity is original, and the problem of the reconstruction of the parent languages of the existing stocks. The tracing of genetic relations between the various stocks is rendered difficult by the lack of written records by which the historical development of the members of the stocks might be determined. Since certain principles of development and differentiation of languages have been derived from the study of other stocks for which we have written records covering long periods, e.g., the Indo-European stock, it might seem permissible to apply these principles to the problems presented by the American Indian stocks. Such a procedure Michelson however regards as erroneous, on the ground that the number of stocks for which we have adequate historical records is too small to give the principles derived from their study universal validity. Another difficulty arises from the fact that it is often impossible to formulate rigorous phonetic laws for the American Indian stocks, because, lacking historical records, the investigator cannot show conclusively what disturbing factors, such as analogy, have been at work. The resemblances between certain stocks raises the question of whether they are due to a remote common origin of the stocks or to borrowings. There is evidence to show that sounds, vocabulary, and syntax may be borrowed. Morphological borrowing has some evidence in its favor, but is still a problem awaiting solution.

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SPECIAL REVIEWS

H. J. WATT. *The Foundations of Music*. Cambridge: University Press, 1919. Pp. xvi + 239.

Watt's volume on *The Psychology of Sound*, published in 1917,¹ has been followed by the book now under consideration. It was obvious that the new theory of consonance and interval which the earlier study brought to light should be tried out in its application to the art of music. In the present volume the application is made with a confidence that leads the author to say in his Preface: "If my results and analysis are valid, the musician should now have a nearly complete and sure basis to work upon, that will give a scientific foundation to all his elementary observations and satisfy him with a sense of firm ground upon which to build" (p. ix).

In order to make the foundations of music intelligible to the musician, the author restates his theory of sound in a clear and well-written summary constituting the first eight chapters. This portion of the book is commended to psychologists who may have been somewhat appalled by the closely written and highly argumentative tone of the earlier work.

The basic conception of tone is that of a "mass or volume of minute [hypothetical] particles of sound sensation, of which those at its centre are the most intense, while the others grade themselves on either side in the whole volume so that the mass appears regular and balanced" (p. 12). In addition, "we have every right to claim that the real basis of our sense of interval is our observation of a constant proportion between the volumes of tones" (p. 41). This brings one to an explanation of fusional *synergy* in terms of the balance established between two or more tones whose volumes overlap, since it is probable that the higher reach of volume is the same for all tones. While the author admits that this last inference is not inevitable, he does not appear to consider the possibility of any two tones of which the volumes do not overlap. Either the upper reach of the volumic spread is identical for all at the point of highest audibility of pitch, or else "it is conceivable that the pyramid of tones should be acute- or obtuse-angled rather than right-angled. But these alternatives are far from likely for various reasons of which the most important will be set forth immediately" (p. 10).

¹ Reviewed in this journal, 1917, 14, pp. 254 ff.

The chief reason appears to be a second assumption regarding the balance of fusing intervals, especially the octave. If pitch is central to volume, the overlapping of volumes will yield a special case when the higher volume lies just midway between the pitch of the lower volume and their common higher terminal, giving only an extra intensity to the upper half, and in the point of predominance that is the pitch of the higher tone. Thus "the whole sound would approximate more nearly to the nature of a single pure tone than would any other form of coincidence of volumes and pitches. These things justify completely the identification of the octave fusion with this special case of overlapping" (p. 19).

It is important to note these assumptions because the validity of Watt's attempt to clear away the mysteries of musical theory is conditional upon their acceptance. Unfortunately, the one bit of experimental evidence we have on the subject seems to run counter to the notion of balance in the octave. The determination of the limen for volume differences by Rich indicates that in the middle region of the scale a just noticeable difference is detected with a constant increment of about .03 of the vibrational frequency. Watt, to be sure, has replied to Rich's contention that such a progression is incompatible with the assumption that the upper of any two tones in octave relation possesses half the volume of the lower; but the interested reader may be left to determine the merits of this controversy for himself.¹

In applying his theory of fusion and interval to musical usage, Watt is forced to emphasize harmonic structure at the expense of simple melody. Thus the major portion of the book is concerned with such troublesome restrictions of musical theory as those that concern "consecutive fifths" and "a fourth from the bass." The conception which emerges from his analysis embraces three types of chords and intervals. These are named, respectively, the "symphonic," the "paraphonic," and the "diaphonic." The first includes the octave, the fifth, and to some extent the fourth; the second comprises thirds and sixths; and the third, all dissonant intervals. What may perhaps be regarded the chief contribution of the present work is the notion of thirds and sixths as "fusionally neutral." Because of their balance the symphonic intervals tend so strongly towards unity of impression that, when repeated, they seem to destroy the onward movement of the melody. On the contrary, thirds and sixths, being "neutral grades of fusion alone do not in succession break this even flow of analytic concentration

¹ Cf. *Jr. of Exper. Psychol.*, 1920, 3, 151 ff.

necessary for the appreciation of the greater works in music" (p. 112). If it be argued that fifths and fourths are tolerable in primitive music, Watt replies that primitive music is homophonic in character, and is, therefore, concerned only with the "horizontal" view of musical structure. Yet without harmony, which is the "perpendicular" complement to the horizontal functions of melody, we have a music which "hardly deserves the name of art. It is merely primitive play, as it were" (p. 161). This extreme conclusion, which appears to deny any system to the musical usage of the non-harmonic scales employed in the Orient, is indicative of the influence a theory may exert over the facts it attempts to organize.

A fourth from the bass is found to be objectionable for a different reason—not because the fourth is too symphonic, but because its tendency is towards paraphony or even diaphony. The fourth owes its position as a true consonance to its "cousinship" with the fifth, but under the conditions in which a fourth from the bass is objectionable it appears as a sort of augmented third, or paraphony, though it may have the character of a diaphony of lesser degree of consonance than the tritone.

But even if we accept the notion of the octave and fifth as balanced and therefore symphonic units of a sort, there appears to be no logical ground for setting off the paraphonic intervals from the diaphonic. Our sense of proportion upon which these distinctions are said to be based, must be remarkable indeed if we can even note the proportional emplacements of the respective volumes and pitch salients of two tones in the relation of a fourth. The unique neutrality of thirds and sixths, as against sevenths and seconds, is still more difficult to comprehend.

Although many counter theories advanced by musicians are carefully examined by Watt, one finds no reference to the suggestive psychological analyses of Lipps and Meyer, or to Bingham's clear formulation of many of their conclusions. According to Watt the *tonic* apparently derives from the fact that the attention falls most easily upon the lowest tone, which thus becomes a point of reference for the tones and chords of a musical unit. We might suppose the lower tone of a sixth or a fourth to be as good a tonic as the lower tone of a fifth, yet a simple experiment readily convinces one that although the trend of the fifth is towards the lower of its tones, that of the true fourth is towards the upper. If tonality is determined by the notes corresponding to 2 or a power of 2 in the ratio numbers of the intervals employed, then the lowest tone is not always and

inevitably the tonic, and a different explanation is suggested for the ban which music has placed upon the employment of consecutive fifths and a fourth from the bass. The ambiguity of key or tonality which Watt rejects as an unsatisfactory explanation of the bad effect produced by fifths gains significance when we know why it is that a new tonality is announced whenever one fifth follows another. If such consecutives are less objectionable when the intervals lie between different voices than when they are between the same voices, this but emphasizes the fact that the departure from a definite tonality is more marked when the same voice is responsible for the break. Similarly the objectionable character of a fourth from the bass is obvious enough if one admits the tonic trend away from the bass note, the tonality of which has hitherto dominated, to the fourth above it which announces a new tonic; for again we have a case of ambiguity and confusion in the musical structure.

To be sure, this conception of the tonic raises the further question of its origin, and implies a mode of solution which Watt rejects—namely, the derivation of harmony and melodic trends from the orderly arrangement of the partial tones. This is not the place to reopen so old and debatable a question, but one may remark that a fact so well known as the tonic trend in melody deserves more consideration than Watt has given it.

The general attitude of the writer in formulating scientific principles for musical art is commendable. His last two chapters, on the "Objectivity of Beauty" and "Aesthetics as a Pure Science," are convincing. In the opinion of the reviewer, Watt's emphasis upon volume in determining our sense of interval and musical form gives us a new conception of sound that must be increasingly dealt with in any attempt to trace the foundations of music. But in the present work the observational data concerning balance among simultaneous and successive tones are too meagre to furnish an adequate foundation for the elaborate superstructure of musical theory which he has reared upon them. As in the elaboration of most theories, the "all or none" principle is unfortunately uppermost; many things like the pattern of pure melody seem to be neglected because they do not readily fall within the conception dominating the author's mind. Thus the assurance with which the facts of musical usage are made to conform to principles which the author has assumed makes one eager to test his assumptions.

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